BIASED POLICY ADVICE FROM THE INTERGOVERNMENTAL PANEL ON CLIMATE CHANGE

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BIASED POLICY ADVICE FROM THE INTERGOVERNMENTAL PANEL ON CLIMATE CHANGE

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At an early stage in their academic training, statisticians are confronted with the Disraeli-Twain notion that there are lies, damned lies, and statistics. The point is not that statistics lie and statisticians are liars. The point is that any fact can be presented in different ways, and that such presentation affects the inference drawn. Statisticians are taught this so that they will not be lured into unsupported conclusions. However, statisticians do not take an Oath of Bernoulli, and some statisticians do use these skills to mislead their audience.

The Fourth Assessment Report (AR4) of Working Group 3 (WG3) of the Intergovernmental Panel on Climate Change (IPCC) is a case in point. Many things can be said about this. Having been involved in AR2 and AR3, and having watched AR4, I cannot escape the impression that WG3 has become more political and less academic, and that overall quality has declined. In some countries, political affiliation seemed to override academic standing as a selection criterion for authorship, while in WG3 the most influential positions went those who tend to support the environmentalists' agenda. Such things are fiendishly hard to prove, and I will not attempt this here. The above is just a personal impression.

I have read most of AR4, and by and large it is able but uninspiring. Climate policy may be one of greatest challenges of our time—it has been 20 years since the IPCC was formed, and 10 years since the Kyoto Protocol was signed, but climate policy has achieved close to nothing—and one would hope that AR4 would teem with intellectual energy in an attempt to solve the many questions that are still open. Instead, it is a rather dull read, with little news even in those areas that I do not follow on a daily basis.

Some things grate. The people that developed the SRES scenarios (Nakicenovic and Swart, 2001) were put in charge of Chapter 3 (Fisher and Nakicenovic, 2007) that evaluates those scenarios. Unsurprisingly, they conclude that they had done a rather splendid job (cf. Gruebler et al., 2004). Unfortunately, they can reach this conclusion only by ignoring large parts of the literature—or, in those cases where referees pointed out the omissions, by listing dissenting papers in the references but not actually using the material. For instance, we read in the SPM that all projections published since SRES have lower numbers for future populations. This is untrue. Fisher et al., (2006) have higher numbers, and this study is referred to by Fisher and Nakicenovic (2007). In fact, Fisher et al., (2006) show that they would get lower population numbers if they

Table 1: Characteristics of emission reduction.

	Target				Base	Costs (%GDP)					
	RF	Concentration		Temp		2030			2050		
	Wm^{-2}	ppm CO_2	ppm CO_{2eq}	°C	# runs	L	M	H	L	M	H
I	2.5–3.0	350–400	445–490	2.0–2.4	6			3.00			5.50
II	3.0–3.5	400–440	490–535	2.4–2.8	18						
III	3.5–4.0	440–485	535–590	2.8–3.2	21	0.60	0.20	2.50	1.30		4.00
IV	4.0–5.0	485–570	590–710	3.2–4.0	118	0.02	-0.60	1.20	0.30	-1.00	2.00
V	5.0–6.0	570–660	710–855	4.0–4.9	9						
VI	6.0–7.5	660–790	855–1130	4.9–6.1	5						

Source: Davidson and Metz (2007), SPM.4–6.

calibrated their fertility model to observations of the last 20 years; but higher numbers if they use data for 50 years.

Procedures were violated too. In its discussion of convergence, Chapter 3 leans heavily on Riahi (2005), which is an unpublished conference paper, unknown to Google, that was in fact severely criticised when presented. Riahi (2005) argues that the implicit assumptions on convergence in the SRES scenarios are in line with theoretical and empirical studies (Adams and Pigliaru, 1999; Barro and Sala-i-Martin, 1992, 1995). Others disagree (Castles and Henderson, 2003,a,b; Dixon and Rimmer, 2006; Holtsmark and Alfsen, 2005; Nordhaus, 2007; Tol, 2006a,b), and part of this material was presented at the same conference.

At two points, WG3 fails to live up to its academic duty. It does not address the issue of the aggregation of different greenhouse gases. Currently, this is done on the basis of Global Warming Potentials (GWP; Forster et al., 2007), which makes little sense from an economic perspective and distorts policy (Kandlikar, 1995; Manne and Richels, 2001; O'Neill, 2000; Reilly and Richels, 1993; Schmalensee, 1993). WG3 does not alert policy makers to the problems and the proposed solutions.

Similarly, Chapter 13 (Gupta and Tirpak, 2007) is rather tame. It discusses policy instruments and international treaties, but it is limited to options, pros and cons. That is fine for ex ante policy advice. However, there is by now some 15 years of experience with actual climate policy—and ex post policy evaluation is more informative than academic discussion about hypothetical policies. Of course, an intergovernmental organisation cannot be as blunt as independent academics can, say on the failure of the Kyoto Protocol (Boehmer-Christiansen and Kellow, 2002; Boehringer, 2002; Manne and Richels, 1999; McKibbin and Wilcoxen, 2004; Nentjes and Klaassen, 2004; Nordhaus, 2006; Nordhaus and Boyer, 1999; Reiner and Jacoby, 2001; Stewart and Wiener, 2003; Tol, 1998) or the incompetence of UK policy (Helm, 2003, 2005; Pearce, 2006). Still, it should not have ignored this literature.

Many people would argue that the core task of WG3 is estimating the costs of greenhouse gas emission reduction. On this, the presentation of findings in Chapter 11 (Barker and Bashmakov, 2007) is odd if not misleading. As an economist, one is struck by the language. The report is structured around emission reduction potentials, as "if we spend Y we get X". It is not structured around costs and targets, as "if we want to achieve X, we need to spend at least Y". That is, WG3 shows inverse demand functions, rather than demand functions. This is not nitpicking. It shows that economists were not in charge in the discussion on costs, their core and exclusive competence.

The numbers shown are misleading. Table 1 amalgamates three tables from the Summary for Policy Makers. Table SPM.4 has background information on six groups of scenarios, but cost estimates are shown for only three of these. The underlying chapter does not provide the missing information. Indeed, it is not obvious how Tables SPM.5 and SPM.6 were constructed, although there are no glaring errors. Figure 1 shows the costs (here, the reduction in the annual economic growth rate) as a function of the target. The curve bends the wrong way. The second partial derivative is negative, not positive. This is at odds with everything we now about emission reduction cost curves (Weyant, 1993; Weyant et al., 2006). The reason for this is

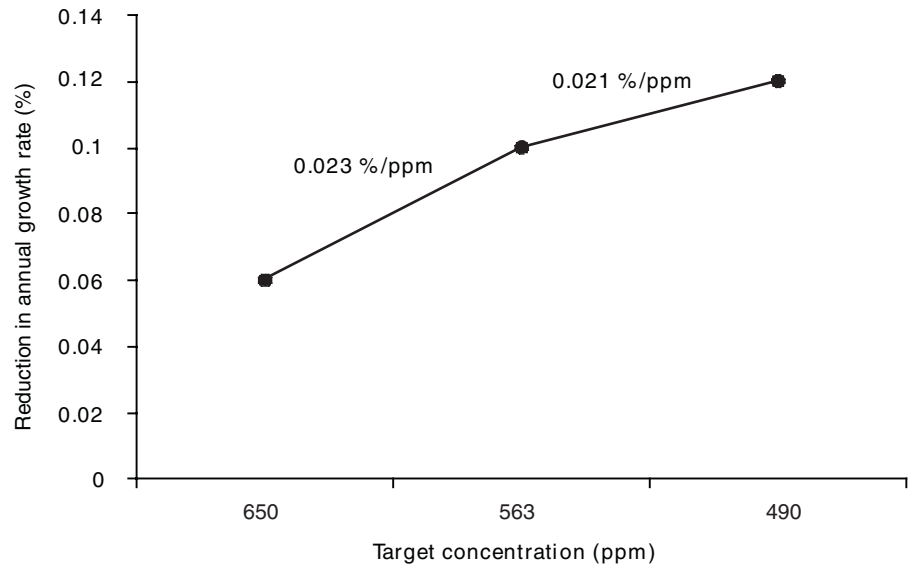

Figure 1: The estimate of the upper bound of the costs of emission reduction (percentage point reduction of the annual growth rate) as a function of the target for the concentration of greenhouse gases (parts per million, CO_2 equivalent). After: Davidson and Metz (2007).

suggested by Table 1. Fewer models report on deep emission reduction targets. Tol (2006c) reports that he tried to reach these targets, but that the model refused. Van Vuuren et al., (2006) report that they could not reach these targets with the standard model, and had to extend the technology vector to get there. Still, they report that a deep target can only be reached from a low baseline. Den Elzen and Meinshausen (2006) highlight that one needs to make rather optimistic assumptions about baseline emissions and the willingness of China and India to reduce their emissions to reach the EU 2°C target. Therefore, it seems safe to conclude that the low number of model results for deep emission reduction targets is not because model runs were not made or reported, but rather because the target is too expensive or technologically infeasible. That is, the sample is censored for the deep target. Therefore, uncorrected sample statistics are meaningless.

This is relevant, because the deepest target of the IPCC is a bit less ambitious than the official target of the European Union (cf. Meinshausen, 2006). AR4 reports costs for meeting this target that are biased downwards, and probably substantially so. It does not report that meeting this target is infeasible in certain models and scenarios, and may therefore be impossible in reality—information which is of critical relevance to policy makers. The IPCC misled by omission.

Related to this, Barker and Bashmakov (2007) conclude that "modelling studies suggest that allowing for endogenous technological change reduces carbon prices as well GDP costs, compared to those in which technological change was largely assumed to be independent of mitigation policies and action". This is a dubious

conclusion. Goulder and Schneider (1999) and Smulder and de Nooij (2003) show that this conclusion only holds in partial equilibrium. Particularly, the models that Barker and Bashmakov (2007) refer to, tend to ignore the opportunity costs of energy R&D. Section 11.5.3 alludes to this literature, but it is ignored in the summary—despite the repeated protests of at least one referee.

For many policy makers, the IPCC reports are the only source of scientific information on climate change. Monopolies are easily seduced into abusing their power. A duopoly may work better, but given the scale of the effort, this may not be feasible. Already too much time and money is spent on assessment of research, rather than research. This implies that the monopoly should be tightly regulated. Although the IPCC purports to be a scientific body, environment ministries have a large say in authors and emphases. There is some attempt to balance this by having representatives of ministries of economic affairs too. This may not be the solution. HM Treasury, for instance, produced a report on climate change (Stern et al., 2006) that is very biased (Dasgupta, 2007; Mendelsohn, 2006; Tol and Yohe, 2006; Yohe and Tol, 2006). It would be much better to shift the IPCC from UNEP and the environment ministries to ICSU and the ministries of research and higher education. Academic quality should be guiding principle in selecting authors. As a check, the committees that nominate and select authors should publish their proceedings. The review editors should become more independent, and gain the right to reject chapters that are not properly revised. The alternative is a gradual erosion of the quality, prestige and, eventually, influence of the IPCC.

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